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ABSTRACT

This paper attempts to promote an understanding of physics through its history and replication of the oscillation experiments of Isaac Newton and Daniel Bernoulli from the 17th and 18th centuries. The experiments described can be treated at a level of sophistication to suit the interests and capabilities of the student. Reproductions in the appendices include: (1) extracts from Newton's "Principia"; (2) essays in the history of mechanics; (3) an English translation of Bernoulli's "Hydrodynamica"; (4) an article on the history of physics; and (5) an article on Newton and fluid mechanics from "Newton Tercentenary Celebrations." (SA)

Historical/Experimental Notes for Newton's Investigation of the Oscillation of Fluids

Experiment No. 4

NEWTON'S INVESTIGATION OF THE OSCILLATIONS OF FLUIDS

(Blue Book pp. 32-39)

Supplementary Notes

(Experiment No. 4)

Contents

Barnard-Columbia History of Physics Laboratory
Samuel Devons
August 1975

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Supplementary Notes

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I. Introduction

Like so many other experiments in the History-of-Physics Laboratory, this one can be treated at a level of sophistication to suit the interests and capabilities of the student.

At the simplest level, it can be regarded as a straightforward exercise in observation and verification of a proposed "law". But the law is so strikingly simple it can hardly fail to evoke some curiosity about its basis. A proposition that engaged Newton's attention, and is worthy of a place in the *Principia* can hardly be trivial. It does, in fact, illustrate the general principles of isochronism involved in what came to be known as "simple harmonic motion" - a general concept which was beginning to emerge in Newton's time.

At a more sophisticated level one can examine, and test, the nature of the assumptions and approximations implicit in the idealized treatment. Such an inquiry will soon stir up questions about the essential properties of a fluid that must be recognized and/or assumed in any attempt to analyse fluid motion on mechanical (Newtonian) principles. (There is no mention of "fluid" in the familiar formulation of Newton's laws!) In a critical vein, one can ask how successful Newton was in his own treatment of the subject; not only in this initial rudimentary problem - which he "solved" - but also in the use he made of it in subsequent developments. And beyond Newton's own inquiries, there were others that attempted to refine and generalize Newton's treatment; to develop, in fact, the whole subject of hydrodynamics from Newton's somewhat haphazard beginnings. This simple experiment with oscillating fluids may not be the most logical starting point for an exploration of fluid-motion, but can provide an opportunity to perceive many of the subtleties that are encountered in any thoroughgoing and precise investigation. Hydrodynamics may have been erected on the firm foundations of Newtonian principles, but it was a long difficult and laborious task to build up the edifice itself. And it is a task which one cannot say is, today, finally completed.

The contrast between the development of classical "rigid-body" and fluid mechanics is particularly illuminating. Both start

out from the base of Newtonian particle mechanics: both have to grapple with the problem of dealing with an essentially infinite number of particles. But in rigid body mechanics one basic assumption - that the mutual separations of all pairs of particles remains constant - suffices to reduce the apparently infinitely complex problem to one which is finite and tractable; and to the extent that the assumption is physically valid precise answers can be given to precisely formulated questions. But in fluid mechanics there is no such drastically simplifying principle. Such idealizations as zero-rigidity (essentially the definition of a "fluid"), zero compressibility and the absence of viscosity greatly help in limiting the range of possibilities, but are far from sufficient to close the huge gap between the limited possibilities in one or two body mechanics, and the immense variety of motions conceivable for a whole continuum. Even when the formal and analytical problems have been mastered, there always remains the problem of how closely the idealizing assumptions correspond to the physical reality in a particular case. In fluid mechanics especially, this may be the hardest question to answer.

Our understanding of mechanical principles starts out from the experience of solids (or solid "particles"). In a world without solids, could our knowledge of the laws of mechanics even have been acquired? And in a world without fluids would we ever have discovered the insufficiency of this knowledge alone in the face of the real complexity of nature?(1)

II. Historical

The sagacious Dr. Thomas Young writes (in 1807):

> It must be confessed, that the labours of Newton added fewer improvements to the doctrines of hydraulics and pneumatics, than to many other departments of science; yet some praise is undeniably due both to his computations and to his experiments relating to these subjects. No person before Newton had theoretically investigated the velocity with which fluids are discharged, and although his first attempt was unsuccessful, and the method which he substituted for it in his second edition is by no means free from objections, yet either of the determinations may be considered in some cases as a convenient approximation; and the observation of the contraction of a stream passing through a simple orifice, which was then new, serves to reconcile them in some measure with each other. His modes of considering the resistance of fluids are far from being perfectly just, yet they have led to results which, with proper corrections, are tolerably accurate; and his determination of the oscillations of fluids, in bent tubes, was a good beginning of the investigation of their alternate motions in general. (2)

Newton is a good yardstick by which to measure the progress and problems in any field of physics. In his Hydrodynamics we are struck, in Samuel Johnson's style, not by how well or badly he performed, but by his attempting the matter at all. We can admire the superb skill and confidence with which he reduces one problem after another to its elements and then provides his elegant solution; we are astonished by the recurrent flashes of insight which enables him to penetrate complexities and invent some simplifying principle; and we are flabbergasted at times, when in some bold (or desperate?) maneuver to rescue himself from difficulties, he makes some wholly unwarranted assumption or discards the very principles on which his whole philosophy is based. His mistakes, like his achievements, are in the grand style! Yet most puzzling is the enormous effort he devoted to this work; and the prominence he gave it in the <u>Principia</u>. It's direct antecedents are hard to find. There was no public debate and lengthy correspondence in the Philosophical Transactions of the Royal Society as there was for his Optics; no record of his lectures on this subject at Cambridge; no elaborate legend of long-nursed ideas as for his gravitational theory. As late as January 1687 - the year of the

publication of the Principia - the members of the Royal Society were still unaware of the extent of Newton's hydrodynamical interests; for

> "Concerning the resistance of the medium to bodies projected through it, as likewise to the fall of bodies",

the Society ordered:

> "That Mr. Newton be consulted whether he designed to treat of the opposition of the medium in it to bodies moving in it in his treatise De Motu Corporum then in the press..." (3)

Though Newton's hydrodynamics appeared to emerge suddenly full-grown in the Principia, the reasons for his concern with these matters certainly has deeper roots. The Principia was not just a new book on mechanics; it was a complete formulation of the mathematical principles of natural philosophy and a new system of the world; not just an extension of existing science, but a complete reformulation. Very much in the spirit of the time, it was essential therefore for Newton to refute prior philosophical systems which his Principia sought to replace. For Newton the heir-presumptive, the occupier of the throne, was Des Cartes. Whether mentioned or not the Cartesian doctrines were an obstacle in Newton's path, and here in the Second Book of the Principia, Newton directs his attack on the central feature of the Cartesian system - its fluids and vortices. Some, at least, of Newton's contemporaries recognized the thrust. Halley's review of the 1st Edition succintly puts the matter thus:

> "From hence is proceeded to the undulation of Fluids, and Laws whereof are laid down, and by them the Motion and Propagation of Light and Sound are explained. The last Section of this Book is concerning the Circular Motion of Fluids, wherein the Nature of their Vortical Motions is considered, and from thence the Cartesian Doctrine of the Vorticals of the Celestial Matter carrying with them the Planets about the Sun, is proved to be altogether impossible." (4)

This alleged refutation of the Cartesian doctrine of vortices is a passing episode in history. Of more lasting significance is the seminal work in hydrodynamics itself, and the manner in which Newton's opposition to DesCartes influenced his attitude in other scientific issues; and thereby the course of the history of physics.

Two great monuments stand to Newton's contribution to science: The Principia (1687) and Opticks (1704); it is natural and commonplace enough to contrast these works in their method, style, origins and influence. The Principia, written in Latin, is formal, austere, mathematical and for the most part deductive. Its mechanical laws, though in principle based on experiment and observation, are certainly not exhibited as the results of Newton's own experiments or inductions from his own observations. The Opticks by contrast is written more colloquially in English; it narrates in full Newton's own experimental investigations, and in its inferences, formal mathematics is generally eschewed. The Principia displays the great mathematical philosopher; the Opticks the superb practical experimenter. Both display Newton the Thinker. One, if not one and the same, Newton, striving ultimately to create a complete natural philosophy which will embrace, or at least, reconcile, his mechanics, his optics and his cosmology; and prepared, or compelled, ultimately to make compromises to this end: to conjecture where experiment does not convince, to postulate what cannot be proved, and even to overlook what cannot be understood.

Yet by-and-large we treat the two Newton books, his mechanics and his optics, as separate. The former we acclaim an unqualified achievement, perhaps the greatest single step forward in the history of science. Towards the latter, our reaction is ambivalent; the beautiful experiments we admire, but to Newton's inferences and his "stubborn" adherence to "particle theory" we commonly attribute a stultifying influence on the science of optics. There is a Good Newton and, if not Bad, then at least a Dangerous Newton.

In Newton's fluid-mechanics (Book II of the Principia) the two Newton's are to be found inextricably interwoven. Here the dichotomy cannot be maintained - or even pretended. The two cerebral hemispheres have to work together. Formal theorizing, and practical experimentation, rigorous deduction and uninhibited speculation, acute observation and (careless?) disregard for facts follow in a rapid succession of seemingly wanton abandon. If the style is, at time, formal - cast in the rigid pattern of Theorems, Propositions and Lemmas - the arguments are anything but rigorous. But Newton's destination is - for himself - quite clear; and he is determined to reach it by whatever path. And astonishingly some of the most extraordinary achievements - for example the "theoretical" interpretation of the velocity of propagation of sound - emerge as evidence of the power of his remarkable genius, even in this confused territory. Book II of the Principia offers us a fascinating view of Newton which lies between his mechanics and optics, and through which are diffused attributes of both - indeed of the whole vast range of Newton's philosophy. On the two firm pillars of

mathematical reasoning and experimental observation he strives to throw a complete, connected arch. It is a perilous structure he erects - lacking the stability of either of its foundations. But it certainly indicates the magnitude and direction of the future tasks.

(For further comments see:
1) by C. Truesdell, Appended p. XII.
2) by the writer, Appended p. XXII.
3) J. C. Hunsaker, on "Newton and Fluid Mechanics", Appended p. XXVII.

References in Text

1) c.f. William Whewell, History of Inductive Sciences, Vol. 2, pp. 116-120. London, 1847.

2) Thomas Young, Natural Philosophy and the Mechanical Arts, Vol. 1. p. 357. London 1807.

3) Birch: History of the Royal Society. (1756). Quoted in Isaac Newton's Papers and Letters on Natural Philosophy (Ed. I. B. Cohen), p. 491. Harvard, 1958.

4) Edmund Halley's Review of the Principia. Philosophical Transactions (291). 1687. Quoted in Ref. 3), p. 409.

For additional references see appended extracts listed above (pp. XV, XXVI, XXVII).

III. Newton's Investigation of Oscillations

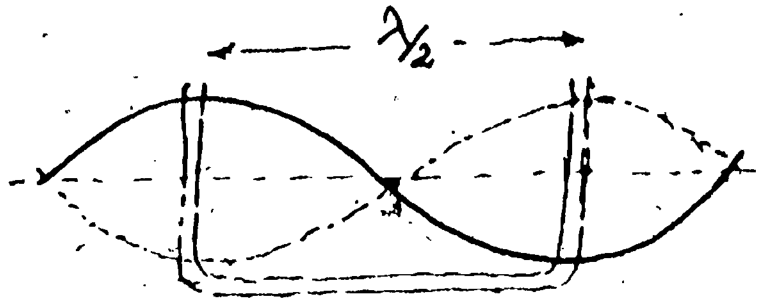
The particular part of the Principia of interest here is Book II, Propositions XLIV to L (Appended pp.I-XI).

After a discussion of the propagation through fluids generally, Newton enunciates his theorem for the oscillations of water (sic) in a U -shape "canal or pipe": Proposition XLIV, Theorem XXXV.

There is no indication that Newton arrived at this theorem as the result of actual observation, or that he experimentally tested the validity of his Theorem, nor indeed of any of the assumptions or approximations which might limit the exact agreement of theory and practice. The pipe is shown in idealized fashion, indicating a uniform cross-section; but with sharply angled bends which could hardly correspond either to a practical arrangement or that most likely to justify the ideal fluid motion which is tacitly assumed. It seems that at this stage Newton has complete confidence in his analytical ability: confirmation of his theory, or the mention of its confirmation, seems superfluous. (Matters are quite different a little later on!)

Newton's "proof" is concise and quite elegant. It is based simply on an analogy between the isochronous motion of a cycloidal pendulum (which is for all practical purposes equivalent to a simple pendulum of small amplitude) and that of the fluid. In both cases the force restoring equilibrium is proportional to the extent of the departure from equilibrium. The proportionality (i.e. the ratio of the force to the mass - which Newton refers to as the "weight"!) for the pendulum is expressed by the ratio of the displacement to the length, as Newton has shown in an earlier part of the Principia (Cor. Prop. LI. Book I.). The corresponding ratio for the fluid oscillations is twice the displacement to the whole length of the fluid column (assumed uniform and in uniform motion). Hence it would be the same for a pendulum of half the length of column (Q.E.D.!).

Immediately following this Proposition, is the Theorem XXXVI concerning the velocity of waves on the surface of water (seeApp. I-V). Newton proceeds with the scantiest of indication of the actual circumstances in which his theory might be applicable, justifying his proceedure only with the casual observation that "the alternate ascent and descent will be analogous to the reciprocal motion of the water in the canal". Proof by analogy! There is some ambiguity in his terminology ("one-oscillation" seems to correspond with one-half of a complete oscillation). His argument and conclusions can be rephrased thus:



The equivalent U-tube extends from one wave crest to one trough. It seems to be assumed (inter alia!) that the height of the crests and troughs is very small compared with the wavelength, λ; and that therefore the "equivalent" pipe is one of length $\lambda/2$.

The isochronous pendulum will then be of length $\lambda/4$ and have a period, τ, $= \pi\sqrt{\lambda/g}$

The wave (phase) velocity ϑ is then:

$$\vartheta = \lambda/\tau = \frac{1}{\pi}\sqrt{\lambda g}$$

This expression is exemplified by Newton's numerical example. No comment is made on its consistency with observation.

That $\vartheta \propto \sqrt{\lambda g}$ can be deduced from dimensional analysis alone; assuming that there are no physical factors other than gravity and inertia, and no dimensional features ("infinite" depth and no lateral boundaries). [Later more realistic theories consider both surface-tension and gravitational sources of energy; and deal with the case of finite lateral dimensions. For very short waves (ripples) it is the surface energy which predominates and Newton's theory is wholly inapplicable.]

One would be surprised if Newton's analogy gave the correct numerical value for the velocity - even when the physical conditions are appropriate. Indeed it does not. The "correct" theory for these long waves gives $1/\sqrt{2\pi} \cdot \sqrt{\lambda g}$, which is about 25% higher than Newton's value. Perhaps one should be astonished how close Newton came to the truth!

From oscillations on the surface of water (an "incompressible fluid") to oscialltions in (elastic) air (Theorem XXXVIII, App.p.V.) is another great step forward. Newton's demonstration of the unmodified-propagation of the wave-form in an elastic medium, and his calculation of the velocity of sound in air seems, to the present-day reader, a tremendous tour-de-force. Using arguments similar to, but far subtler than that used for the U-tube oscillations; (with no formal calculus, no differential equations!), he demonstrates that a simple-harmonic motion (represented by the projection of uniform circular motion on a line) of each infinitesimal segment of the medium is consistent with the physical conditions, in which the pressure varies inversely as the volume. (Newton refers,

throughout, to the propagation of "the pulses"; but it is, of course, simple sinusoidal oscillations of each particle, i.e. a wave motion of a single frequency, that he is dealing with. The "pulses" refer to the motions of each infinitesimal segment.) He eventually arrives at the result that the velocity of propagation of (longitudinal) compressional waves is equal to [elastic compressibility of the air/ density of air]$^{1/2}$. Numerically he finds this velocity, V_s, is equivalent to:

$$V_s = 2\pi (H/\tau_H),$$

where H is the height of a column of air (of the same density as that through which the sound is propagated) of equal weight to the barometer height, and τ_H is the period of a simple-pendulum (an isochronous cycloidal one, for Newton!) of length H.

In modern terminology we would write for the velocity of sound,

$$V_s = \sqrt{K/\rho}$$

where K is the elasticity of the air and ρ its density.

For isothermal compression and dilation (which is what Newton tacitly assumes)

$$K = -V\frac{\partial p}{\partial V} = p, \text{ (the pressure) };$$

and with $$p = \rho g H ,$$

then $$V_s = \sqrt{gH}$$

Using for g, the result $\tau_H = 2\pi\sqrt{\frac{H}{g}}$, or $\sqrt{g} = \frac{2\pi\sqrt{H}}{\tau_H}$,

We obtain Newton's result: $V_s = 2\pi(H/\tau_H)$

Newton quotes the values 29,725 ft. for H (compare 27,800 for dry air at 15°C); and $T_H = 190\frac{3}{4}$ (equivalent to g = 32.2 ft/sec and thus, $V_s \approx 979$ ft/sec.

Compared to the experimental value (Sauveur: standing waves in pipes) of 1142 ft/sec, the agreement is none too good! But Newton's imaginative resourses are not exhausted. Even if it means jettisoning his most cherished principle - that forces act between

the ultimate particles, which are themselves hard, solid and immutable - he is determined to make theory and experiment meet. With his incredible notion of the "crassitude of the particles", (See Appendix p. X; c.f. also p.XV) and some additional patching up by appeal to the moisture-vapour in the air, he manages to produce a theoretical value - 1142 ft/sec - identical with experiment! This wild conjecture is, of course, utterly wrong. It is the implicit use of isothermal elasticity (proportional to p; from the "law" pV=constant) rather than its adiabatic value (γp) that leads to the discrepancy with theory. Newton's analysis of the propagation of sound is essentially correct without any patchwork. As LaPlace commented, one hundred years later: "His theory, although imperfect, is a monument to his genius."

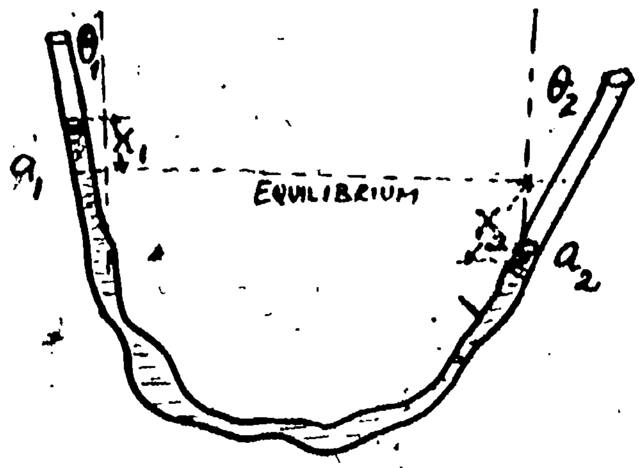
In a few pages of the Principia we can span the whole range of Newton's remarkable work: from an assured mastery in applying his formal, logically worked-out theory to the bold exploratory speculations in a domain where great imagination is as significant as logical analysis. His analysis of the U-tube problem lies nicely as a link between the two.

IV. Daniel Bernoulli's Generalizations

We return to Newton's treatment of the U-tube oscillations as a problem in hydrodynamics. Apart from the idealization of a "perfect" incompressible, frictionless fluid, there is the implicit assumption that all parts of the fluid system move together harmoniously, that is to say, the relative velocities of different parts of the fluid remain constant throughout the motion. The kinetic energy of the fluid can then be written as Av^2 ; the product of the square of the velocity of any convenient part (e.g. the fluid in the vertical limbs) and A a constant of the whole arrangement. Since the P.E. is proportional to the square of the displacement from equilibrium, Bx^2, this ensures that motion is simple-harmonic, and of period $\tau = 2\pi\sqrt{A/B}$.

Daniel Bernoulli in his Hydrodynamics (1736) attempts to generalize the argument beyond Newton's example of a U-tube of uniform bore and vertical limbs: first to a uniform pipe with sides inclined to the vertical, then to a more general arrangement (See appended extract from Hydrodynamics; pp.XVI). Bernoulli's treatment of the problem is based on the principle of vis-viva (or "live-forces" as it is translated!) whose modern counterpart is the energy principle. The nature of Bernoulli's "solution", which is somewhat buried in his elaborate notation, can be illustrated by a slightly more specific example. We consider a pipe of varying cross-section, which is nevertheless constant at each fluid surface over the full extent of the oscillations. The potential energy is then:

$$P.E = \tfrac{1}{2} g\rho\,(a_1 x_1^2 \cos\theta_1 + a_2 x_2^2 \cos\theta_2);$$



and since the fluid is assumed incompressible; $a_1x_1 = a_2x_2$.

$$P.E = \tfrac{1}{2} g\rho\, a_1 x_1^2 (\cos\theta_1 + a_1/a_2 . \cos\theta_2)$$

If the fluid motion is assumed to be everywhere parallel to the "axis" of the pipe, so that for each element of length δl having a cross-section a_l the K.E. can be expressed as: $\tfrac{1}{2}\rho\, a_l v_l^2 \delta l$, the total K.E. can be expressed thus:

$$K.E. = \tfrac{1}{2}\rho \int a_l v_l^2\, dl$$

$$= \tfrac{1}{2}\rho \int a_l (a_1/a_l \cdot \dot{x}_1)^2\, dl$$

$$= \tfrac{1}{2}\rho\, a_1^2\, \dot{x}_1^2 \cdot L/\bar{\bar{a}} \,,$$

$\bar{a} = L \Big/ \int \frac{dl}{a_l}$ is a "mean" cross-section over the whole length L. Now if the pipe is not uniform the value of $\bar{a}$ will change as the position of the fluid changes, (the limits of the integral for the K.E. over dl change!); but for small oscillations —

$$a_1 x_1 \ (= a_2 x_2) << \bar{a}\, L$$

$\bar{a}$ can be assumed constant; and the K.E. $\propto x_1^2$.

From these expressions for K.E. and P.E. we than have for period:

$$T = 2\pi \cdot \left\{ (L\, a_1 a_2) / g\bar{a}(a_2 \mathrm{Cos}\,\theta_1 + a_1 \mathrm{Cos}\,\theta_2) \right\}^{\frac{1}{2}}$$
$$= 2\pi \cdot \left\{ \frac{L}{g} \frac{a_1 a_2}{\bar{a}(a_2 \mathrm{Cos}\,\theta_1 + a_1 \mathrm{Cos}\,\theta_2)} \right\}^{\frac{1}{2}} ;$$

(which of course reduces to Newton's result for $a_1 = a_2 = \bar{a}$; $\theta_1 = \theta_2 = 0$)

The above limitation to small oscillations is simply a consequence of the geometry of the pipe. But even if the pipe is of uniform cross-section, there is an implied physical assumption that the pattern of the flow stays constant throughout the oscillation. But since the speed of the fluid flow is varying (approximately sinusoidally), the Reynold's number is changing and the pattern may well be different in different phases of the motion. If this is so the conditions for simple-harmonic motion will not be satisfied. The isochronism of the oscillations, for different amplitudes, is then dependent on both factors - the geometrical and the physical. This can, of course, be tested experimentally.

V. **Damping of the Oscillations**

Neither Newton nor Bernoulli discuss the damping of these oscillations (frictionless fluid). Provided this is small ($T_{damping} \gg T_{oscill}$), the effect on the frequency will be small (of the order: $(T_{oscill}/T_{damp})^2$). However as the oscillations die down, both the frequency and the damping may change; the former for the reasons already given, the latter because the pattern of the flow and the relative significance of viscosity and turbulance may change.

A separate study of the damping does reveal that the decrease of amplitude is not precisely exponential. The departure from exponential damping will of course depend on the size/shape of the pipe and the nature of the fluid: for example sharp "corners" will introduce relatively large Reynolds' numbers: $V \cdot \rho / d\eta$ (η is viscosity, and ρ density of the fluid: V and d respectively the characteristic velocity and transverse dimensions of the fluid motion): Or the fluid velocities which range from zero to some maximum value in each cycle, may, especially near the angularities, pass through the critical value where the nature of flow changes "abruptly".

VI. Experiments

1. The apparatus (for details see pp.16/17) comprises several U-tubes of various shapes, some with (colored) water others with mercury. The larger-bore U-tubes are suitable for water, and may be filled with varying amounts. One U-tube is mounted on a separate board which may be oriented at any desired angle to the vertical. Oscillations can be stimulated easily by means of the small pneumatic bulb. The simple pendulum alongside the U-tubes may be readily adjusted in length.

2. Start with a U-tube with vertical sides. Set the pendulum oscillating (small amplitude!) and then set up the oscillations of the liquid in the U-tube. Notice, simply by eye, which oscillation is more rapid; then adjust the length of the pendulum and repeat the observation. Measure the length of the pendulum which seems closest to isochronous with the U-tube oscillations.

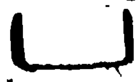
Measure the overall length of the liquid column as accurately as you can - along the "axis" of the pipe. With this pendulum length fixed, examine the isochronism for various <u>amplitudes</u> of oscillation of the liquid in the U-tube. Verify that over a limited range, the oscillations are isochronous.

3. A more precise method of matching the pendulum and liquid oscillation periods is by counting 'beats'. When the two oscillations are not quite isochronous, one can watch the oscillations start by being in step, then go out of step, then come back into step. Count the number of pendulum oscillations (n) for one such cycle. By tabulating n - or better 1/n - for different pendulum lengths (and denoting n as positive/negative according as the pendulum is faster/slower), one can easily <u>interpolate</u> to estimate the pendulum length for which 1/n is zero - which is the condition for isochronism.

4. Repeat these measurements for several different amounts of liquid in the U-tube. (Note: it is easier to add water, than to remove it.) Tabulate your results:

i) Length of water column
ii) Length of isochronous pendulum
iii) Ratio of ii)/i)

5. Repeat the measurements for the U-tube with mercury (Do <u>NOT</u> attempt to change the amount of mercury in the tube!). Compare the result here with those in #4.

6. Compare the isochronous pendulum-lengths for two different tubes of the sort:  and 

7. Determine the isochronous pendulum-length for the tube of the sort:  and 

8. Determine the isochronous pendulum-lengths for the U-tube which can be rotated, with a constant amount of fluid, but for different angles of orientation. Tabulate:

i) $\cos\theta$ (θ is angle between limbs and vertical)

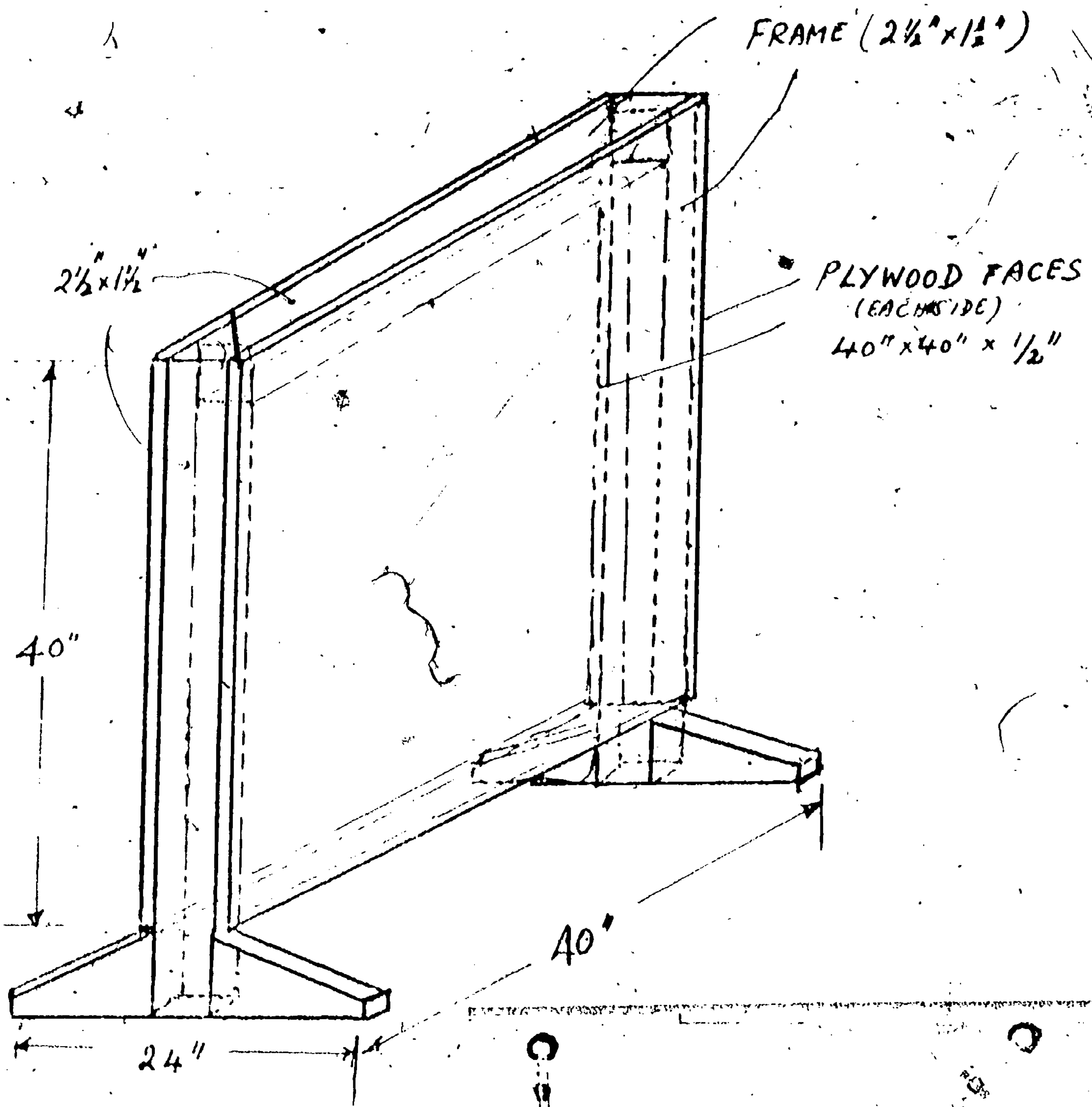
ii) Length of the isochronous pendulum L

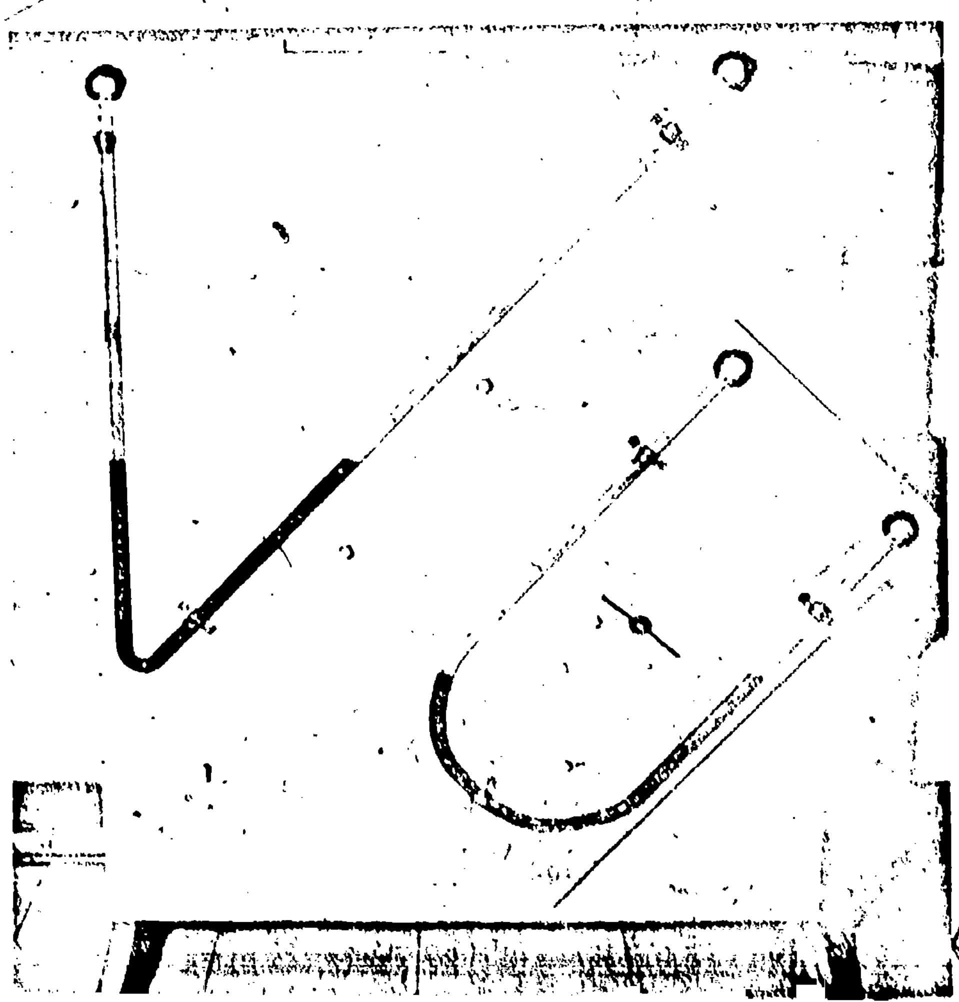
iii) 1/L

What do you notice?

9. Additional investigations may be made of the damping of the oscillations for both water and mercury.

NEWTON: OSCILLATION of FLUIDS : FRAME





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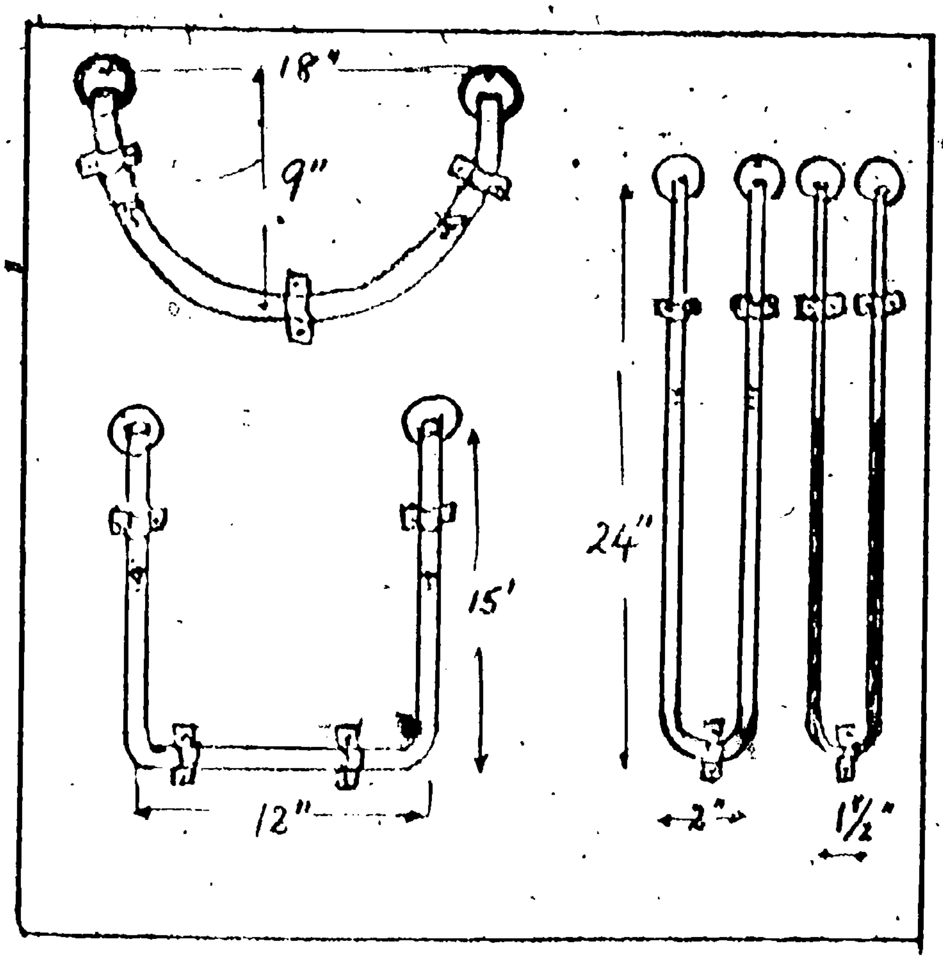
NEWTON : OSCILLATION of FLUIDS. TYPICAL TUBE ARRANGEMENTS



FRONT

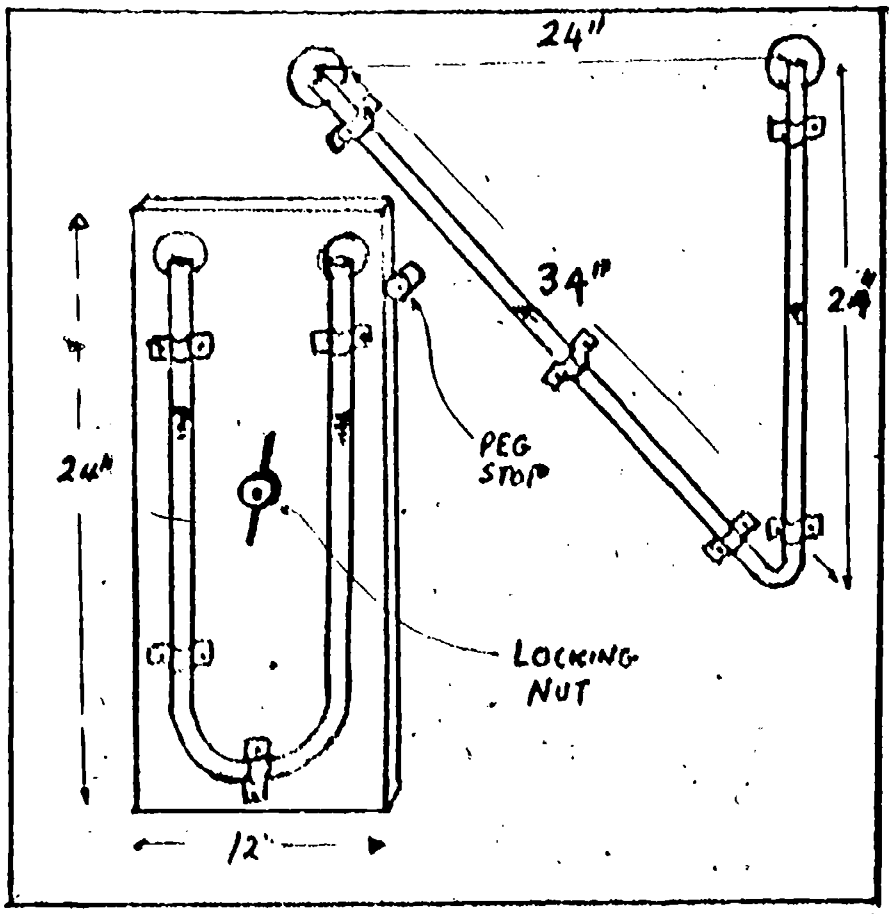
3/16" I.D. (for Mercury)

TYPICAL GLASS TUBE DIMENSIONS:

For WATER : ½" I.D.

For MERCURY 3/16" I.D.

Holes, 1½" Diam. (for Access to tube ends)



BACK

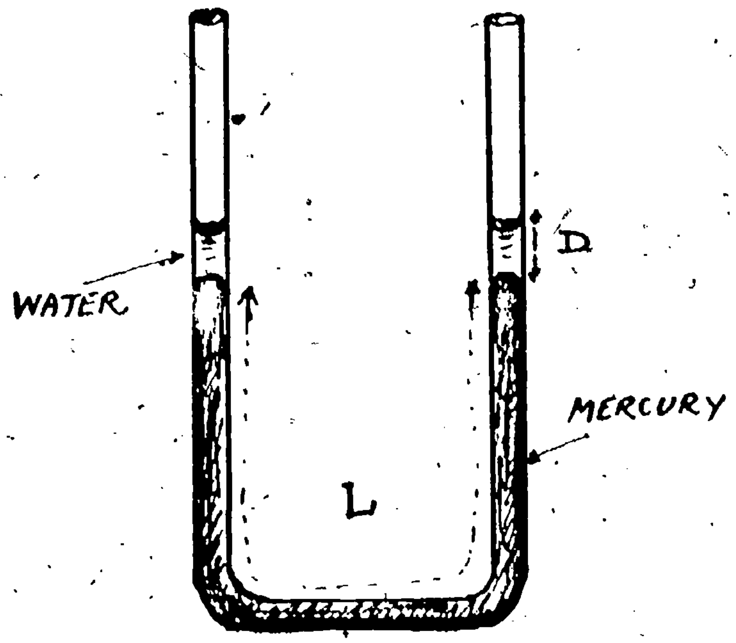
VII. Some Questions

1. Newton states the theorem for water. Do you find the same theorem true for mercury (which is nearly 14 times denser than water)? Explain - in words! - why the density of the fluid does, or does not, influence the period of oscillation.

2. The oscillations are strongly "damped". How would you expect this to depend on the bore of the U-tube? Why are the U-tubes used for water of larger bore than those used for mercury?

3. Would Newton's theorem be valid if the bore of the U-tube were not constant throughout its length? If the limbs of the U-tube were not straight?

4. Suppose a hybrid system -- as shown -- were used (Do NOT attempt to reproduce this system in the laboratory!). What would you expect to be the length of the isochronous pendulum?



5. Can you give a simple interpretation of the oscillation period when the U-tube was not vertical?

6. With what accuracy have you verified Newton's Theorem; and how accurately do you think Newton might have verified it?

7. Do you think Newton was sufficiently confident of his theoretical analysis not to concern himself about an experimental verification? Verification of this Theorem might have been regarded (at the time) as a test of Newtonian Principles. Would it have been a particularly severe or crucial test? What particular features of Newtonian theory would be tested? (c.f. question #1 above)

10.—Understanding Physics Through Its History. By Samuel Devons, F.R.S., Department of Physics, Columbia University. (With 2 text-figures)

(1)

It is a great pleasure to join in this tribute to Professor Norman Feather: My recollections of him as teacher, friend and colleague extend back nearly forty years. I recall attending, as a Cambridge undergraduate, his lectures on 'Properties of Matter' (what an archaic and nostalgic flavour that title has now!); and hearing him describe his pioneering experiments on the properties of the neutron. He was my doctoral thesis examiner; and later when he was Editor of the Cambridge monographs, it was at his suggestion, and with his help and encouragement, that I made my first essay as an author. I was privileged to succeed my former teachers, Professor C. D. Ellis and Professor Feather, at Trinity College; and in the 1950s I was regularly and warmly welcomed at Edinburgh, the ambivalent benevolence of my role as 'External Examiner' notwithstanding.

My most recent contact with Professor Feather has been one of which he may be unaware—consulting his engaging series of introductory surveys of the principles of (mainly) classical physics [1]. There is the recurrent implication in the treatment of many topics in these books, that an understanding of science is, or can be, enhanced by some knowledge of its historical development. This issue is not forcibly belaboured; but the conviction is quietly and characteristically affirmed, both by example and by the modest declaration of purpose, to present 'a carefully told story, starting at the beginning'. It is on this matter of the function and value of 'history' in teaching or learning physics, and on a particular way of introducing its perspective in the laboratory, that I would like to offer a few comments, and an illustration.

It is not my purpose to expatiate on the value of studying the history of science as a means of understanding the function or limitations of science, of science as a part or a product of evolving culture, or the role of science in contemporary civilisation. No one seriously contends that for an understanding of these general features of science as a whole, some knowledge of its history is not essential. What is more debatable—but less frequently debated—is the value of explicit historical studies for the understanding of a particular topic in science itself: its present structure, methods, range and concepts. Amongst those who teach physics there is a fairly marked cleavage, in practice and in precept, between those who adopt the formal-theoretical, largely deductive and often mathematical approach, and those who take a more phenomenological, inductive quasi- (or pseudo-) historical path. Since physics is not—even today—a wholly deductive science, and certainly not a totally empirical one, both approaches are valid, but neither can claim to present a complete picture. But many will assert that in its present state of maturity, physics can largely dispense with the historical-inductive approach. And judging by most of today's textbooks, they do just that. I have no basic quarrel with this decision nor a desire to convert those who have taken [illegible] I do ask whether the student, who receives *all* his instruction from teachers of this persuasion, is not being deprived of some opportunity to enhance his understanding of the subject? Might it not be that if the subject were, even occasionally, illuminated from a different direction, the new perspective and bolder relief would enhance the value of what is already learned?

I have elsewhere [2] elaborated this advocacy of an occasionally-different viewpoint in studying physics, and particularly how this might be accomplished in an experimental laboratory. The association of history with the laboratory, rather than the lecture room or library, is usually regarded, at the outset, as rather eccentric. But why should it be so? After all, it is experiment and observation that constitute the empirical-phenomenological component of physics and it is this aspect which is so close to history. Moreover, new ideas as well as new facts are often born in the laboratory; and it is by examining them in their nascent state that their fuller (and later) meaning can often be better understood. It is not simply a question of studying how physics was created—as distinct from what it now is—but rather by re-creating one may understand better what has evolved. The standard counter to this argument is that life is too short to retrace the whole evolution of ideas, concepts and theories of a subject with such an immense history as physics. And the periodical reassessments and reformulations of the whole structure of the subject, in which older ideas are absorbed or discarded, and which historically have occurred from time to time, make all this unnecessary. This argument would be unanswerable if it were possible to present the whole contemporary conceptual framework of physics, with all its sophistication, to the beginner starting out to master the subject. But in practice there are steps to be taken on the way. Simple ideas and simple matters (or what are portrayed as such!) have to be presented first. No one, no matter what his approach, starts out by discussing the problems of contemporary physics: invariably one begins with simpler questions, most of which were studied, and resolved, in the past.

An experimental laboratory in which the viewpoint is explicitly historical has one cardinal advantage over other historical approaches. One studies the *phenomena*, and the phenomena never lie; nor do the phenomena themselves change with time. Of course, the phenomena which are studied, those which command the attention of the exploring scientist, change dramatically. But a student faced with experimental phenomena which have puzzled scientists, whether of 100 or 200 or 300 years ago, is faced with a physical situation as 'real' as it ever was. If, in his attempts to interpret and understand what he observes, he adopts a consciously historical approach, it is only in so far as he does not use concepts, instruments or techniques which did not exist at the time, and which perhaps *could* not have existed prior to the elucidation of the problem he is studying. In experiment, observation and interpretation, history and logic are not so disjoint as one might imagine. And as with any more orthodox approach to a physical problem, some background, and a basis from which to start, must be assumed and provided. Here it is the historical-scientific context which provides this base for a particular experimental enquiry; and the enquiry is an exercise within a specified framework. If it be argued that such a framework is 'unreal' (meaning not the full context of contemporary physics), then we might ask which 'exercises' are not so defined? Solving some problem involving point masses or infinite conducting planes is not less 'artificial'.

When, three or four years ago, a start was made on such an historical-experimental laboratory, it seemed obvious that the most appropriate would be some of the 'great

experiments' that we now regard as the landmarks in physics. Many such experiments, for example those associated with the names of Gilbert, Newton, Franklin, Coulomb, Ampere, Faraday, Joule, Hertz, etc., have been reconstructed, and do indeed provide excellent exercises both in experiment and in interpretation; and when performed with a proper knowledge of their historical context, also provide some insight into how physics comes into being, as well as what it now is. But that is not the issue at stake here. Experience soon demonstrated that not only the famous experiments but many others--some famous, some forgotten, some successful, some failures—stepping stones rather than mile-stones in the development of physics, can be equally illuminating when repeated in the proper historical context. In brief, *every* experiment is presented as a *problem*: Can it be understood in terms of pre-existing concepts and principles? If so, how? If not, what new concepts and principles are suggested? This is a matter of *how* physics works. And if adoption of a deliberately historical approach rarely fails to evoke the question of *why* a particular problem or phenomena is of interest at all, who would be so churlish as to deny that this, also, is part of one's understanding?

The example that follows, one of the more elementary ones in our history-of-physics laboratory, is illustrative of only some features. It is certainly not typical: in fact, experiments in mechanics, as a class, rarely are. Classical mechanics has its roots more in observation than in experiment; so that most such laboratory experiments tend to be illustrative rather than exploratory. In this case it is not even certain that the experiment was ever performed by its illustrious author. But it surely marks a significant step in the development of his ideas; and it soon leads to a confrontation of theory with experiment which is far from trifling. And as an experiment to verify some preformulated ideas, rather than to discover new phenomena, it is far from unusual in the history of physics. One conclusion above all is soon reached in an historical laboratory: style, aim, and function of experiments of every variety have played a part in the development of physics. There is no 'typical' experiment in physics.

-XXIII-

II. Newton's Investigations of the Oscillations of Fluids

1. *The background to Newton's work on fluid motion*

Every student of science has heard of the great Isaac Newton (1642–1727) and his monumental work 'The Principia' (*Philosophiae Naturalis Principia Mathematica*, 1686—First Edition). One learns that this unique, epochal book provided the first major synthesis of the principles of physics; that these principles formed the foundation of Classical (Newtonian) mechanics which in turn provided a firm bedrock for further advances in physical science for more than two centuries.

The first part of 'The Principia' formulates these powerful new principles and laws: the concepts of mass and force, the laws of motion and the Universal Law of Gravitation. Newton himself, of course, exploits these new principles to construct his own 'System of the World (in mathematical treatment)', which forms the substance of Book III of 'The Principia'. Logically one might expect the laws and principles to be conceived first, and then their application to various features of the universe at large follow; and although this is the formal order of presentation in 'The Principia', it would be misleading to infer that such a neat logical order was generally followed in

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Newton's time, or by Newton himself. At this stage in the evolution of science, whole new ways of examining and understanding the physical universe are being created and examined, and in this context there could be no sharp separation between principles and their application. The adoption of a particular cosmological viewpoint both helps to find and formulate principles and is, in turn, shaped by them.

In the actual writing of 'The Principia' (1st Edn: 1687)—a task which Newton accomplished in months, although most of what he wrote had occupied his mind over a period of more than twenty years—Newton, not unnaturally, attempts to exhibit a logical sequence. The nature of this sequence is stated by Newton himself with exemplary clarity in the opening paragraph of Book III, viz.:

> 'In the preceding books I have laid down the principles of philosophy; principles not philosophical but mathematical: such, namely, as we may build our reasonings upon in philosophical inquiries. These principles are the laws and conditions of certain motions, and powers or forces, which chiefly have respect to philosophy; but, lest they should have appeared of themselves dry and barren, I have illustrated them here and there with some philosophical scholiums, giving an account of such things as are of more general nature, and which philosophy seems chiefly to be founded on; such as the density and the resistance of bodies, spaces void of all bodies, and the motion of light and sounds. It remains that, from the same principles, I now demonstrate the frame of the System of the World. Upon this subject I had, indeed, composed the third Book in a popular method, that it might be read by many; but afterwards, considering that such as had not sufficiently entered into the principles could not easily discern the strength of the consequences, nor lay aside the prejudices to which they had been many years accustomed, therefore, to prevent the disputes which might be raised upon such accounts, I chose to reduce the substance of this Book into the form of Propositions (in the mathematical way), which should be read by those only who had first made themselves masters of the principles established in the preceding Books: not that I would advise anyone to the previous study of every Proposition of those Books; for they abound with such as might cost too much time, even to readers of good mathematical learning. It is enough if one carefully reads the Definitions, the Laws of Motion, and the first three sections of the first Book. He may then pass on to this Book, and consult such of the remaining Propositions of the first two Books, as the references in this, and his occasions, shall require.'

Although Newton refers to 'the preceding books' (i.e. Books I and II of 'The Principia'), it is clear from the later remarks that it is mainly on Book I and its Definitions, Axioms, Laws and Mathematical demonstrations (all of which concern 'the Motion of Bodies' through empty space) that the 'System of the World' of Book III rests. These principles and their extraordinarily successful application are unquestionably the outstanding achievements of 'The Principia' and the source of Newton's powerful influence over successive generations of natural philosophy. It is for this accomplishment that Newton was, and is, universally extolled. But interposed between Book I and Book III, and occupying more than a quarter of the whole work, there are matters of a rather different sort—considerations of 'The Motion of Bodies in Resistive Mediums'—which although partly resting on the principles developed in Book I, are little exploited in the application of these principles in Book III. To be sure, the form of 25

Book II bears much superficial resemblance to the development of the laws of mechanics in Book I—there are the familiar Propositions, Theorems, Lemmas, Problems and Scholiums; yet the nature of the subject, its state of development in Newton's time and Newton's own contributions are of a very different nature. Here Newton is not at all the great synthesiser—producing order, system and reason in much that is already known, and more than he himself creates. Book II, unlike Book I, is not the spectacular climax to decades, and centuries, of observation, speculation and theorising. Rather it is an attempt to begin—even if falteringly—a new science. Many of the arguments and methods Newton uses here are sketchy, uncertain improvisations, often sheer guesswork; and as such many are incorrect. They are of interest as much because they are Newton's as for their intrinsic merit. And even here Newton's genius is impressive: he demonstrates how powerfully he is able to develop new concepts, modes of investigation and analysis and to reach definite conclusions in questions whose apparent complexity and obduracy must have deterred his predecessors and contemporaries. Newton may not always have proceeded in the correct way—but before his work there was virtually nothing!* (For a contemporary assessment of Newton's work on fluid mechanics see [5].)

Why did Newton expend so much effort on this topic at all? And why was it given so important a place—directly between the fundamentals of Book I: 'The Motion of Bodies', and Book III: 'Systems of the World'? Today one might regard Book III as the logical sequence to Book I and skip Book II entirely (as many who refer to 'The Principia' implicitly do!).† If this is so, it is surely because some of the major, and in Newton's day revolutionary and even heretical (in the scientific sense) ideas, have now become wholly commonplace. Especially is this true of the basic concepts of the motion of objects (the heavenly bodies) through a void, guided by an abstract, mathematically defined, attractive influence (universal gravitation), spanning space with no, or no specified, intervening agency. Whatever may be today's view of the fundamental validity of these Newtonian concepts, they are neither startlingly unfamiliar nor do they provoke violent controversy. But matters stood very differently in Newton's time.

Isaac Newton grew up at a time when the new Natural and especially Mathematical Philosophy was dominated by the influence of Descartes (1596–1650) and the 'Cartesians'. Many of the ideas and principles which appear in the Newtonian system of Mechanics derive from Descartes' writings (e.g. Newton's first law of motion—the 'Law of Inertia'—had been earlier formulated in almost exactly the same words by Descartes). However, there are profound 'philosophical' differences between the Newtonians and the Cartesians regarding the nature of space and the possibility of influences acting through a void. The philosophical conflict is not only deep; its partisans are widespread and the dispute goes on for decades. These differences appear wholly irreconcilable in their respective cosmic systems and interpretations of the planetary orbits. To Voltaire, who visits England in 1727—the year Newton died—the contrast is still striking:

* Nothing, that is to say, in the way of serious investigations of fluids and *motion*. There was, of course, a considerable legacy of work in hydro*statics*, extending from antiquity (e.g. Archimedes' treatise on 'Floating Bodies', *c*. 250 B.C.) to Newton's time (e.g. Pascal's treatise on the Equilibrium of Liquids and the Heaviness of the Mass of Air, 1663).

† Newton, were he alive today, might well echo Heine's complaint—that many praised his work

'A Frenchman who arrives in London, finds a great difference in philosophy as in other things. He left the world full, he finds it empty. At Paris you see the world composed of vortices of subtle matter, in London we see nothing of the kind. . . .'

Newton is clearly preparing to meet the expected criticism and prejudices of his powerful opponents—the followers of Descartes. To do so he must enter their territory; and not only enter it but master the very subject—motion in and of fluids—which they introduce so cavalierly to 'explain' the motion of the heavenly bodies. To Newton it is not, apparently, enough to present a rival theory, even one which is far more powerful in its interpretive value; it is also necessary to demolish the theory of one's rivals. It is not so much then a question of what place Newton's 'digression' into fluid motions is relevant to his *own* System of the World; but of the need to demonstrate that it cannot form the basis of an alternative one.

This whole question of motion in empty space (and the concomitant 'action-at-a-distance') is an older and wider issue than the conflict between Cartesian and Newtonian theories of the motion of the Planets. Motion in a vacuum is still, in Newton's time, regarded by many as an unreal—or at best an abstract—concept. The ancient principle that 'nature abhors a vacuum' still exerted its influence on philosophic thinking, and a theory in which such motion-in-a-void occupies a central role could be challenged on the grounds of its metaphysical unreality and the utter impossibility of its demonstration. So unreal was the existence of the vacuum to Aristotle that he bases his explanation of motion—by interaction with the medium traversed—on the principle of its non-existence. Aristotelianism did not die a sudden death with Galileo. To many of Newton's contemporaries, the idea of motion (and forces) in a void was a return to the mystical and the occult, and even those opponents who did not genuinely feel so could still use it as a stick with which to beat the Newtonians. Descartes, although he rejects the Aristotelian explanation of uniform motion in favour of the principles of inertia, still retains the principle that all influences which *change* an object's motion must be due essentially to direct contact of their impenetrable parts. Not only the properties of space, but also the nature of force, is wholly different in Cartesian and Newtonian philosophies. ('Among you Cartesians, all is done by an impulsion which one does not well understand; with the Newtonians it is done by an attraction of which we know the cause no better'—Voltaire 1727 again.)

But this does not imply—as Newton attempts to show in Book II of 'The Principia'—that the properties of a medium full of (fluid) material, and the motion of a (solid) object through such a medium are outside the scope of his System; but rather that they occupy a very different role from that assigned to such phenomena either by the Cartesians, or the Aristotelians before them.

Having become involved, for whatever motive, in this subject of motions in fluids, Newton does not stop at dealing *only* with those aspects which relate to his rival's System. With characteristic vigour, imagination and ingenuity Newton directs his energies successively to a whole range of problems, creating on the way new concepts and new methods of analysis. From the motion of objects *through* fluids, it is for him, but a short step to the motion *of* fluids—steady motion through pipes, orifices and canals, the oscillatory motion of waves on the surface, the propagation of sound, and the nature of the spring (elasticity) of fluids, and so on. It is in the course of these

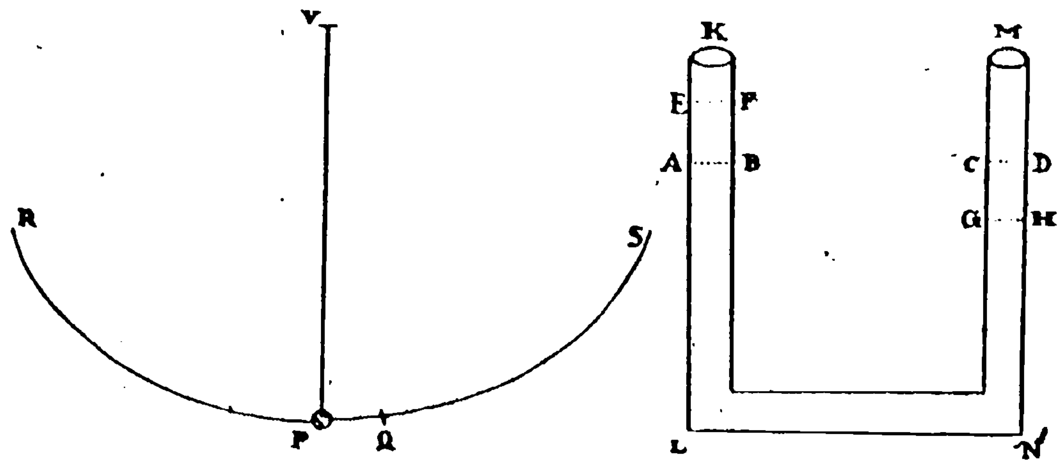
enquiries that Newton treats the problem of the oscillations of a fluid in a vertical U-tube. This is best presented in Newton's own words [3].

2. *Newton's Proposition XLIV, Theorem XXXV (Book II)*

'If water ascend and descend alternately in the erected legs KL, MN of a canal or pipe; and a pendulum be constructed whose length between the point of suspension and the centre of oscillation is equal to half the length of the water in the canal: I say, that the water will ascend and descend in the same times in which the pendulum oscillates.

'I measure the length of the water along the axes of the canal and its legs, and make it equal to the sum of those axes; and take no notice of the resistance of the water arising from its attrition by the sides of the canal. Let, therefore, AB, CD represent the mean height of the water in both legs; and when the water in leg KL ascends to the height EF, the water will descend in the leg MN to the height GH. Let P be a pendulous body; VP the thread, V the point of suspension, RPQS the

-XXV-



cycloid which the pendulum describes, P its lowest point, PQ an arc equal to the height AE. The force with which the motion of the water is accelerated and retarded alternately is the excess of the weight of the water in one leg above the weight in the other; and, therefore, when the water in the leg KL ascends to EF, and in the other leg descends to GH, that force is double the weight of the water EABF, and therefore is to the weight of the whole water as AE or PQ to VP or PR. The force also with which the body P is accelerated or retarded in any place, as Q, of a cycloid, is (by Cor., Prop. LI, Book I) to its whole weight as its distance PQ from the lowest place P to the length PR of the cyclid. Therefore the motive forces of the water and pendulum, describing the equal spaces AE, PQ, are as the weights to be moved; and therefore if the water and pendulum are quiescent at first, those forces will move them in equal times, and will cause them to go and return together with a reciprocal motion. Q.E.D.'

Cor. I. Therefore the reciprocations of the water in ascending and descending are all performed in equal times, whether the motion be more or less intense or less.

Cor. II. If the length of the whole water in the canal be of 6 1/9 feet of *French* ... will descend in one second of time, and will ascend in another

second, and so on by turns *in infinitum*; for a pendulum of 3 1/18 feet in length will oscillate in one second of time.

'Cor. III. But if the length of the water be increased or diminished, the time of the reciprocation will be increased or diminished as the square root of the length.'

Newton's 'proof', as you can see, is extremely concise and really very elegant. He shows that the motion of the fluid in the tube is a dynamical analogue of another, already solved, problem. This is the problem of the 'simple' pendulum (i.e. a 'point' mass oscillating at the end of a 'weightless' string) which was, in Newton's time, a familiar and well-studied one. It is part of the folk-lore of physics that Galileo was inspired by watching the swinging lantern in the Duomo at Pisa to conceive the idea of isochronous oscillations. This subject was taken up by Christian Huygens (1629–1695) and Christopher Wren (1623–1723), both of whom are referred to in 'The Principia' as having successfully 'solved' this problem. Newton himself devotes many pages in 'The Principia' to what he terms '. . . oscillating pendulous motion of bodies' (Section X of Book I). His reference is to motion along a 'cycloid' which he has shown, using powerful (but now archaic) geometrical arguments—characteristic of 'The Principia'—to be truly isochronous motion for any amplitude of oscillation. An 'ordinary' simple pendulum, oscillating along the arc of a circle, approximates to the cycloidal oscillations if the maximum angle of the arc is small.

The essential feature of such ideal oscillations—'simple harmonic motion' in today's terminology—is that in any part of the cycle, the force-per-unit-mass acting on the displaced object, and therefore its acceleration, is proportional to its displacement from the place of equilibrium. If, then, two types of oscillatory motion—no matter how they differ in other respects—are both characterised by the feature of proportionality, with the same value of the constant of proportionality, they will have the *same period*. It is such a direct similarity argument that Newton invokes here.

Newton does not indicate whether, or how, he has examined such oscillations experimentally; or whether he knows or has attempted to verify the truth of his 'Theorem XXV'. Possibly this was already a well-known fact? Notice, also, that this oscillation is essentially an example of one-dimensional motion, for which the application of Newtonian principles is straight-forward and unequivocal.

3. *The relation of Proposition XLIV, Theorem XXXV to Newton's other work*

Having once started an examination of the oscillations of fluids, Newton certainly does not stop with this simple example. He goes on to face a much more challenging one—the general problem of the transmissions of vibrations through fluid media of indefinite extent: Waves on the surface of water, sound vibrations through the air, etc., a subject which has already been tentatively explored. Newton's calculations and speculations here are of great interest in the history of physics for several reasons. Firstly, Newton's is the first successful quantitative explanation of the velocity of sound in terms of the *static* properties of air. This work demonstrates his powerful grasp of the *principles* involved in wave propagation generally—an understanding which is sufficient for him to see the inherent difficulties in applying these same principles to the interpretation of *light* as a wave propagated through a medium. Newton's reluctance to accept a wave theory of light is all too well known; but cer-

tainly not well understood. For Newton's contemporaries who had no theory at all (in the Newtonian sense) of wave propagation in a medium, there must have been little difficulty in postulating some such wave motion for light. Newton knew too much to accept such an ill-defined, and what he must have considered careless and ill-thought-out, association. Where ignorance was bliss, perhaps it was frustrating to be too wise! In any event, Newton's opposition to a wave theory of light was to influence physics for a century or more after him; and it was many decades before his arguments against such a theory could be properly understood and answered.

This simple theorem about the oscillations of water in a tube is then of intriguing interest, in that it is a link between the formal precise arguments that characterise Newton's great contributions to mechanics of particles on the one hand, and on the other his exploratory and more speculative work on the motions and oscillations in fluid media, and the nature of sound and light. It is interesting to contrast the manner in which Newton, having demonstrated his Theorem for the oscillations of fluid in the U-tube, proceeds (in 'The Principia') without hesitation to the next 'theorem'—about the velocity of waves on water. Already rigorous analysis is replaced by intuitive plausibility. And a few bold steps later—when he has completed his intricate theory of sound propagation, he stops to compare his theory with observation. And he does not find everything quite in order. One bold speculation leads to another; and we finally observe an ingenious (or ingenuous!), and quite incorrect, attempt to reconcile theory and experiment.* In a few pages we can span the whole range of Newton's remarkable work: from an assured mastery in applying his formal, logically worked-out theory to bold exploratory speculations in a new domain where great imagination is as important as logical analysis. His analysis of the U-tube problem lies nicely as a link between the two.

The influence of Newton's exploration of the motion of fluids on his successors may have been less profound than his major achievements in mechanics and cosmology, but it was none the less of major significance. Fifty years after the publication of 'The Principia', there appeared the celebrated work of Daniel Bernoulli: *Hydrodynamica* (1737) [4]. In the section entitled 'Concerning the Oscillation of Fluids in Curved Tubes', we find:

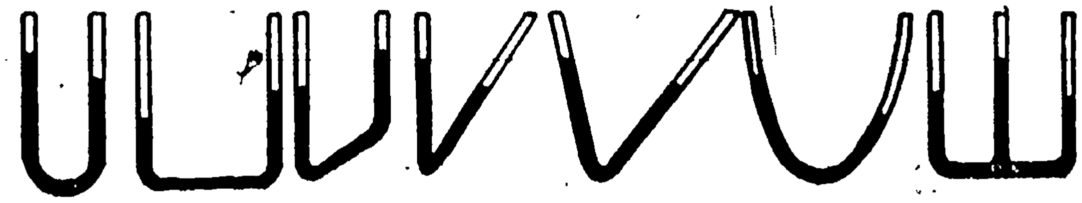
> 'These are the things which have been communicated to the public up to this time on the oscillations of fluids, and certainly first by Newton, in order to show the nature of waves, . . .'

—a clear example of the seminal significance of Newton's contribution to hydrodynamics, and its lasting influence.

III

The foregoing represents an attempt to provide an introductory-historical background to some simple experiments on the oscillation of fluids in pipes—Newton's original U-tube and simple variations and extensions of it. With the minimum of explicit instructions, the student is asked to verify Newton's theorem, and to examine its extension in such arrangements as:

* See [3], esp. pp. 383–384.



a b c d e f g

Both mercury and (coloured) water are used as fluids. Oscillations with composite fluid columns (e.g. water on mercury) can also be studied. The only 'instrument' is a ruler; no timepiece is necessary.

Many questions are posed, to stimulate both experiment and interpretation. For example:

> Newton states the theorem for water. Is it true for mercury which has a density nearly 14 times greater? Could you have predicted this? What principle(s) of Newtonian mechanics are involved here? Is this experiment a sensitive or precise test of these principles? What better evidence for these already existed at the time?
>
> Is the oscillation truly isochronous (independent of amplitude)? Would this be so if the bore of the tube were not uniform? What do you observe in arrangement (f)?
>
> Is there a unique period for the arrangement (g)? Does the solution of this problem really afford a solution to the problem of water waves (Theorem XXXVI), as Newton affirms?
>
> Might the theorem 'The velocity of the waves varies as the square root of the breadths' be correct, although Newton's numerical estimates are not?
>
> The bore of the U-tubes with mercury is smaller than those with water. Why? What essential physical properties of *fluids* are assumed in the analysis of all these problems? How are these assumptions tested in this simple experiment?

For some appreciation of the subsequent development of this subject the student is referred to (and reproductions of short extracts are appended for encouragement!) selected passages from 'The Principia' [3], Daniel Bernoulli's *Hydrodynamica* [4], and a modern commentary on Newton's attempt to solve hydrodynamical problems [5]. And a student (with limited analytical equipment) who would like to see, in more detail, how different and more subtle continuum mechanics is than one-particle mechanics, can be referred to Professor Feather's excellent elementary analysis of water waves, in the work mentioned at the beginning [6].

ACKNOWLEDGMENT

I would like to thank Professor A. Ziggelaar of the Royal Danish School of Educational Studies for some helpful comments.

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NEWTON AND FLUID MECHANICS

By PROFESSOR J. C. HUNSAKER

Newton's conception of dynamics armed his successors of the next three centuries with basic tools with which they erected the great structures of modern engineering science.

While Newton is claimed by mathematicians and astronomers as their own, engineers owe to him the very foundation of their art. Modern aerodynamics is completely Newtonian in its development. Not only is it a consequence of Newton's laws of motion, but to this day it continues to utilize some of his original tactics to obtain solutions.

For example, Newton, being unable to determine from first principles the resistance of a body moving through a real fluid, simplified his problem by separating it into three parts. He postulated, first, a special frictionless incompressible fluid, then a viscous fluid, and finally a compressible fluid.

For the frictionless fluid, he deduced that there would be a resistance, due to the impact of fluid particles, varying as the fluid density, as the square of a linear dimension of the body and as the square of the velocity of motion.

For the second fluid, having 'a want of lubricity' or viscosity, he concluded that the frictional force must vary as the rate of shear of adjacent layers of fluid. This is a clear definition of the coefficient of viscosity and the basic characteristic of laminar flow. To this day we speak of tars and greases as non-Newtonian fluids because they do not exhibit this linear relation between force and rate of shear.

Finally, he speculated on the propagation of pressure pulses, like sound waves, in a compressible fluid and found the velocity of propagation to be a function of the density and elasticity of the fluid. His computation for the velocity of sound in air was a fair approximation which stood until Laplace corrected it for adiabatic rather than isothermal conditions.

From the concept of the ideal frictionless fluid comes the classical hydromechanics of Bernoulli, Euler, d'Alembert, and Lagrange.

From the concept of the viscous fluid we have the great development by Stokes and Osborne Reynolds leading to Prandtl's boundary layer mechanics.

From the concept of compressibility, we are just now evolving a comprehensive mechanics of supersonic flow to cope with the engineering needs of modern flight.

Newton's ideas are as old as reason and as new as research. Their timelessness is shown by three simple instances.

Newton clearly stated that his laws of motion imply that it does not matter whether a body be fixed and the fluid flow over it, or whether the body move through a fluid at rest. This kind of relativity is the justification for the wind tunnel testing by the Wright brothers and their followers in aerodynamics research.

A second example of old and new is the modern wing theory which accounts for lift as the reaction to the force which communicates downward momentum continuously to the air. Froude applied the same reasoning to marine propulsion.

The third instance is the newest style of all, jet propulsion. Jet propulsion as a device of mechanics is certainly Newtonian in principle. The propulsion is the third law reaction to the second law force, as measured by the rate at which momentum is created in the jet. While Newton is said to have speculated about a jet-propelled steam carriage, certain marine creatures were making use of a propulsive jet very much earlier and might claim priority in geological time.

From: Newton Tercentenary Celebrations (1946). Cambridge, 1947. pp. 82-83.